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Running Head: ANTAGONISTIC EFFECTS ON IMPLICIT MEASURES

When the Method Makes a Difference: Antagonistic Effects on "Automatic Evaluations" as a Function of Task Characteristics of the Measure

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Abstract

Researchers often employ implicit measures as dependent variables to investigate processes of attitude formation and change. In such studies, experimentally induced differences are typically interpreted as reflecting change in automatic evaluations. We argue that experimentally induced effects on implicit measures may not always reflect genuine changes in evaluative responses, but can be driven by the mechanisms underlying the measurement procedure. In line with this assumption, the present research shows that these mechanisms can produce opposite effects of the same experimental manipulation for otherwise equivalent implicit measures. These results indicate that merely observing experimental effects on implicit measures does not allow direct inferences regarding changes in automatic evaluations. Instead, psychological interpretations of such effects hinge upon the mechanics of how a given measurement procedure responds to variations in the context. Implications for research using implicit measures are discussed.

Keywords: Affect Misattribution; Affective Priming; Automatic Evaluation; Implicit Measures; Response Interference

When the Method Makes a Difference: Antagonistic Effects on "Automatic Evaluations" as a Function of Task Characteristics of the Measure

Experimental procedures to measure automatic evaluations or immediate affective responses gained immense interest during the last 10 years, and are currently used as standard tools in a variety of research areas (for reviews, see Fazio & Olson, 2003; Petty, Fazio, & Briñol, in press; Wittenbrink & Schwarz, 2007). The core feature of these measures is that they provide an index of people's propensity to respond favorably or unfavorably to a given stimulus without requiring an explicit evaluation of that stimulus. More precisely, such "implicit" measures allow one to infer evaluations from response latencies or error rates, typically in speeded categorization tasks. The most prominent examples of these measures are Greenwald, McGhee, and Schwartz's (1998) Implicit Association Test (IAT) and Fazio, Jackson, Dunton, and Williams' (1995) Bona Fide Pipeline (BFP).[1] Other examples include Payne, Cheng, Govorun, and Stewart's (2005) Affect Misattribution Procedure (AMP), De Houwer's (2003a) Extrinsic Affective Simon Task (EAST), and Nosek and Banaji's (2001) Go/No-go Association Task (GNAT).

Based on evidence that automatic evaluations assessed by these measures reliably predict judgments and behavior (for a review, see Fazio & Olson, 2003), researchers became increasingly interested in questions pertaining to their origin and change (Gawronski & Bodenhausen, 2006; Petty, Briñol, & DeMarree, 2007; Rudman, 2004; Wilson, Lindsey, & Schooler, 2000). For instance, with regard to their origin, researchers have explored the roles of evaluative conditioning (e.g., Olson, & Fazio, 2001), cognitive balance (e.g., Gawronski, Walther & Blank, 2005a), and ingroup favoritism (e.g., Otten & Wentura, 1999). Moreover, research addressing change in automatic evaluations has investigated various mechanisms, including cognitive dissonance (e.g., Gawronski & Strack, 2004), attitude-related education programs (e.g., Rudman, Ashmore, & Gary, 2001), and extended training in negating evaluative responses (e.g., Gawronski, Deutsch, Mbirkou, Seibt, & Strack, 2008a; Kawakami, Dovidio, Moll, & Hermsen, 2000). Taken together, these results suggest that (a) automatic evaluations may be more malleable than suggested by some models (cf. Wilson et al., 2000), and (b) the mechanisms underlying their formation and change may differ, at least partially, from the ones previously obtained for self-reported explicit evaluations (for a review, see Gawronski & Bodenhausen, 2006).

In the present article, we suggest that researchers should be cautious in interpreting experimentally induced effects on implicit measures as direct evidence for changes in automatic evaluations. In line with earlier warnings (e.g., Eder, Hommel, & De Houwer, 2007; Fazio & Olson, 2003; Klauer & Musch 2003; von Hippel, 2004), we argue that performance on indirect measurement procedures does not provide direct reflections of the evaluations they are designed to assess. Instead, evaluations influence task performance only indirectly by means of further psychological processes that mediate the link between the two (Brendl, Markman, & Messner, 2001; Conrey, Sherman, Gawronski, Hugenberg, & Groom, 2005; De Houwer, 2003b; Klauer & Musch, 2003; Rothermund & Wentura, 2004). To be sure, the operation of task-specific mediators does not necessarily challenge the validity or usefulness of implicit measures in predicting judgments and behavior. It does, however, imply a more complex picture of the apparent flexibility that has previously been observed with implicit measures. Specifically, we argue that experimental manipulations may not only influence automatic evaluations proper, but also the task-specific processes that mediate between evaluation and task performance (see Figure 1). Thus, experimental effects on implicit measures can be due to either (a) a genuine change in the underlying evaluative response (see Figure 1, Path A), or (b) a secondary change in the task-specific mediator (see Figure 1, Path B). More seriously, if a given manipulation influences task-specific mediators rather than underlying evaluations, the same experimental manipulation can lead to different outcomes for different kinds of measurement procedures. Needless to say, such experimental effects on task-specific mediators have the potential to distort any theorizing about automatic evaluations, if these effects are mistakenly interpreted as reflecting genuine changes in automatic evaluations. In the present studies, we provide evidence showing that different task-specific mediators can even lead to opposite effects resulting from the same experimental manipulation, pointing to the significance of these issues if the role of task-specific mediators is neglected.

Variants of Affective Priming

Even though our claims are applicable to any kind of implicit measure, the present research is particularly concerned with two variants of affective priming, namely Fazio et al.'s (1995) BFP and Payne et al.'s (2005) AMP. In general, affective priming tasks are based on the notion that the processing of a target stimulus is influenced by the valence of a prime stimulus that is briefly presented before the target. Depending on the particular paradigm, affective priming mainly occurs in the form of compatibility or assimilation effects (for a review, see Klauer & Musch, 2003). Important to the present discussion, such priming effects can be mediated by different task-specific mechanisms, which have their roots in procedural details of the respective measures.

Bona Fide Pipeline

The most prominent affective priming paradigm is Fazio et al.'s (1995) BFP. In this task, participants are asked to make speeded evaluative decisions about positive and negative target words, which are preceded by positive or negative prime stimuli. Affective priming effects in this paradigm are reflected in compatibility effects, such that prime-target pairs of matching valence result in faster and more accurate target-evaluations than non-matching pairs (for reviews, see Fazio, 2001; Klauer & Musch, 2003). Originally, researchers assumed that affective priming effects in the BFP resemble the non-affective spread of activation in associative networks (e.g., Fazio, Sanbonmatsu, Powell, & Kardes, 1986; Hermans, De Houwer, & Eelen, 1994; see Collins & Loftus, 1975). That is, initial stimulus evaluations were thought to result in an increased activation of evaluatively congruent material in memory, which in turn was assumed to facilitate the encoding of evaluatively congruent targets and to interfere with the encoding of evaluatively incongruent targets. Available evidence supports the operation of this encoding-related mediator in the BFP, though its actual influence turned out to be minor compared to other mechanisms (Klauer, Musch, & Eder, 2005).

Over the past decade, an accumulating body of research provided evidence that response-interference (RI) rather than spreading activation may be the dominant mediator of priming effects in the BFP (e.g., De Houwer, Hermans, Rothermund, & Wentura, 2002; Gawronski, Deutsch, & Seidel, 2005b; Klauer & Musch, 2002; Klauer, Roßnagel, & Musch, 1997; Klauer & Teige-Mocigemba, 2007; Klinger, Burton, & Pitts, 2000; Wentura, 1999). To illustrate the RI mechanism, assume that a participant was instructed to respond with a right (left) hand key-press to positive (negative) target words. According to the RI account, the short-term associations implied by these instructions are sufficient for both primes and targets to trigger the respective response tendencies (De Houwer, 2003b). That is, both positive primes and positive targets trigger a response tendency to press the right key, whereas both negative primes and negative targets trigger a response tendency to press the left key. Thus, if primes and targets are of different valence, the two stimuli trigger two different response tendencies, thereby interfering with quick and accurate responses to the target. In contrast, if primes and targets share the same valence, the two stimuli elicit the same response tendency, thereby facilitating quick and accurate responses to the target.

Affect Misattribution Procedure

Payne et al. (2005) recently introduced an affective priming variant, the AMP, which resembles the BFP on the surface, but substantially differs in methodological details and therefore in its task-specific mediator. In this paradigm, participants are briefly presented with a positive or a negative prime stimulus, which is followed by a neutral Chinese character (see also Murphy & Zajonc, 1993). After a brief interval, the Chinese character is replaced by a masking stimulus, and participants are asked to indicate whether they consider the Chinese character as more or less pleasant than average. Affective priming in this paradigm is reflected in assimilation effects, such that the neutral Chinese ideographs are evaluated more positively (negatively) when they were preceded by a positive (negative) prime stimulus (Payne et al., 2005). The AMP procedure differs from the BFP in several ways, the most important differences being that the targets in the AMP are of neutral valence, semantically meaningless for participants, presented very briefly, and are replaced by a masking stimulus. In the BFP, on the other hand, the targets are of clear semantic meaning and valence, and typically remain on the screen until participants responded.

To account for priming effects in the AMP, Payne et al. (2005) suggested a misattribution mechanism whereby the affect elicited by the prime is (mistakenly) used to evaluate the Chinese character. Such misattribution processes are likely facilitated by several features of the task, such as the lack of a clear evaluative or semantic meaning of the target stimuli, their brief and single presentations, as well as their replacement by a masking stimulus. Importantly, these characteristics also eliminate RI as a potential mechanism in the AMP. Given that the target stimuli in the AMP lack a clear evaluative and semantic meaning, they are unlikely to trigger the same kind of response tendencies as the target words in the BFP. As such, there are no response tendencies elicited by the targets that could be congruent or incongruent with the response tendencies elicited by the primes, thereby undermining the occurrence of RI. Instead, it seems likely that the affect elicited by the prime prolongs during the presentation of the Chinese character, thereby biasing participants' evaluations of the target. Thus, as Payne et al. (2005) argued, participants seem to mistakenly assume that their affective reaction stems from the target character, which may result from their inability to disentangle the relative contributions of prime-related versus target-related responses to their momentary affective state (Wilson & Brekke, 1994).

Despite these differences in task-specific mediators, it seems reasonable to assume that the BFP and the AMP still tap the same automatic evaluations. This assumption is supported by research showing corresponding effects for the two measures, such as for example their shared moderation by motivational variables in the prediction of self-reported evaluations (e.g., Fazio et al., 1995; Payne et al., 2005). At the same time, the aforementioned procedural differences may make them differentially susceptible to the same experimental manipulation, if this manipulation influences their task-specific mediators (see Figure 1).

Construct-Related vs. Method-Related Variations

Separating method-specific and construct-specific effects is certainly not an easy task. A useful example to illustrate this difficulty is a recent study by Gawronski et al. (2005b). Their experiments examined variations in automatic evaluations of positive and negative stimuli when these stimuli are encountered in the context of a positive or negative stimulus. Employing the basic structure of the BFP, participants were first presented with a context stimulus of either positive or negative valence, which was replaced by a positive or negative prime stimulus (Balota & Paul, 1996). This sequence was followed by the presentation of a positive or negative target word, which had to be classified in terms of its valence. Results indicated that affective priming effects of a given prime stimulus were more pronounced when this stimulus was preceded by a context stimulus of the opposite valence than when it was preceded by a context stimulus of the same valence. In other words, positive prime stimuli facilitated positive responses to a greater extent when they appeared in a negative rather than a positive context. Likewise, negative prime stimuli facilitated negative responses to a greater extent when they appeared in a positive rather than a negative context.

At first glance, one may conclude that these contrast effects reveal a general principle of automatic evaluations. In line with the principle of hedonic contrast (Brickman, Coates, & Janoff-Bulman, 1978), one could argue that affective reactions to a given stimulus are generally enhanced in a context of the opposite valence, whereas affective reactions tend to be reduced in a context of the same valence. On the other hand, the observed contrast effects could also be explained by the operation of RI mechanisms in the BFP. Drawing on earlier research showing visual contrast effects in attention (see Cacioppo, Crites, Berntson, & Coles, 1993; Gawronski, Deutsch, & Strack, 2005c), Gawronski et al. (2005b) argued that context stimuli of the opposite valence increase the salience of the valence of the subsequent prime, whereas the salience of prime valence should be reduced by context stimuli of the same valence. Such attentional effects seem particularly important in the BFP, given previous evidence showing that priming effects in this task depend on participants' attention to the primes (e.g., Musch & Klauer, 2001; Simmons

& Prentice, 2006; Spruyt, De Houwer, Hermans, & Eelen, 2007; see also Proctor & Cho, 2006). In terms of the RI account, one could argue that increased salience of prime valence increases the activation of a pre-potent response tendency elicited by the prime, thereby enhancing the size of RI effects (Gawronski, Deutsch, & LeBel, & Peters, in press). Importantly, to the degree that evaluative features of an object may influence automatic evaluations even when perceivers do not pay attention to those features (e.g., Cunningham, Raye, & Johnson, 2004), the RI mechanism underlying the BFP may sometimes produce attention-related variations in measurement scores that do not reflect genuine variations in automatic evaluations. At the same time, it is also possible that attention to evaluative attributes of an object modulates automatic evaluations of that object (Fazio, 2007). In this case, the attentional mechanism proposed by Gawronski et al. (2005b) may actually produce genuine variations in automatic evaluations, rather than spurious variations resulting from the task-specific mediator. Based on these considerations, contextual contrast effects on BFP scores may reflect either construct-related effects on automatic evaluations or method-related effects on the task-specific mediator (see Figure 1).

The Present Research

To test the potential involvement of method-specific processes in experimentally induced effects on affective priming, the present research employed two strategies. First, we compared experimental effects on two affective priming paradigms: one that is based on RI (BFP) and one that is based on misattribution (AMP). To the degree that our manipulation differentially influences the task-specific mediators underlying these measures, we expected substantial differences in their outcomes even when the two measures were identical with regard to the employed stimuli and presentation times. For this purpose, Experiments 1 and 2 compared affective priming effects resulting from two sequentially presented prime stimuli in Fazio et al.'s (1995) BFP (Experiment 1) and Payne et al.'s (2005) AMP (Experiment 2). In Experiment 1, we aimed at replicating the contextual contrast effects observed by Gawronski et al. (2005b), which served as a standard of comparison for Experiment 2. If contextual contrast effects in the BFP are driven by genuine changes in automatic evaluations, these changes should occur irrespective of whether the employed measure does (BFP; Experiment 1) or does not (AMP; Experiment 2) involve an RI component. If, however, the obtained contrast effects were driven by the impact of differential salience of prime valence on RI effects, evaluative context primes should lead to contrast effects only in tasks that do involve an RI component (BFP; Experiment 1), but not in tasks that do not involve an RI component (AMP; Experiment 2). To the contrary, given that the misattribution mechanism of the AMP has been shown to integrate independent sources of affect (Murphy, Monahan, & Zajonc, 1995), the AMP may show additive rather than contrast effects of evaluative context stimuli. Such additive effects would be in direct opposition to the contrast effects in the BFP.

The second strategy employed in the present research was to test whether the observed effects are limited to measures of evaluative responses. Based on the dissociation predicted for Experiments 1 and 2, one could still object that the contrast effect in the BFP is a genuine characteristic of automatic evaluations that cannot be captured by the misattribution mechanism of the AMP. To rule out this concern, we aimed at replicating the obtained dissociation for non-evaluative, semantic variants of the two paradigms. Given that semantic priming should follow principles of spreading activation rather than hedonic contrast (e.g., Balota & Paul, 1996; see Collins & Loftus, 1975), the contrast effects obtained for the evaluative variant of the BFP should turn into additive effects for the non-evaluative, semantic variant of the BFP, if these contrast effects reflect a genuine characteristic of automatic evaluation. If, however, the obtained contrast effects are caused by method-specific rather than construct-related factors, they should also occur if the same measurement procedure is used to assess non-evaluative responses (Eder et al., 2007). For this purpose, Experiments 3 and 4 employed non-evaluative variants of Fazio et al.'s (1995) BFP and Payne et al.'s (2005) AMP using the same general set-up and timing as in the first two experiments. However, deviating from the measures employed in Experiments 1 and 2, the two tasks were designed to measure the activation of the semantic categories *animate* versus *inanimate*. In Experiment 3, participants completed a variant of the BFP involving animate or inanimate context stimuli, which were followed by either animate or inanimate prime stimuli. The prime stimuli were then replaced by animate or inanimate target words, and participants' task was to indicate as quickly as possible whether the target word depicts an animate or an inanimate object (non-evaluative RI task). In Experiment 4, participants were primed with the same stimuli used in Experiment 3, but were presented with neutral Chinese characters as target stimuli. Participants' task was to guess whether the Chinese character refers to an animate or an inanimate object (non-evaluative judgment). If the contrast effects obtained for the BFP are indeed driven by method-related rather than construct-related factors, the non-evaluative paradigms employed in Experiments 3 and 4 should reveal the same paradigm-related dissociation, even when the response dimension is non-evaluative rather than evaluative. Such a finding would provide further evidence for our assumption that method-specific psychological processes can be responsible for experimentally induced effects on implicit measures.

To isolate the effects of measurement paradigm (BFP vs. AMP) and type of response (evaluative vs. non-evaluative), identical context and prime stimuli were used across the four experiments. Hence, although Experiments 1 and 2 were primarily concerned with affective priming effects, the stimulus materials also varied in terms of the non-evaluative dimension employed in Experiments 3 and 4 (i.e., animate vs. inanimate). Likewise, although Experiments 3 and 4 were primarily concerned with non-evaluative priming effects, the stimulus materials also varied in terms of valence (i.e., positive vs. negative).

Experiment 1

The primary goal of Experiment 1 was to demonstrate contextual contrast effects resulting from two sequentially presented prime stimuli in an implicit measure that does contain a RI component, namely Fazio et al.'s (1995) BFP. Participants were presented with positive or negative target words, which were preceded by context and prime stimuli varying in terms of valence. Participants' task was to indicate as quickly as possible whether the target word depicts a positive or a negative object. Based on earlier findings by Gawronski et al. (2005b), we expected the priming effects of positive and negative primes to be more pronounced when they appeared in an evaluatively incongruent context than when they were presented in an evaluatively congruent context.

Method

Participants and design. Fifty-four undergraduates at the University of Western Ontario (40 female, 14 male) participated in Experiment 1. All subjects received course credit for their participation. The experiment represented a 2 (First Prime Valence: positive vs. negative) × 2 (First Prime Category: animate vs. inanimate) × 2 (Second Prime Valence: positive vs. negative) × 2 (Second Prime Category: animate vs. inanimate) × 2 (Target Valence: positive vs. negative) × 2 (Target Category: animate vs. inanimate) within-subjects design.

Materials. We used 15 words of each positive animate objects, negative animate objects, positive inanimate objects, and negative inanimate objects, which were selected via pre-tests (see

Appendix). Each set of words was divided into three subsets, resulting in three sets of five stimuli for each of the four stimulus categories. The three subsets were used as first primes, second primes, and target stimuli in the BFP. The particular position of the subsets (i.e., first prime, second prime, target) was counterbalanced across the experimental conditions.

Procedure. The priming tasks consisted of 128 trials, including two trials for each of the 64 possible combinations of first prime, second prime, and target implied by the aforementioned experimental manipulations. Each trial began with the presentation of a blank screen for 700 ms, followed by a fixation cross (+) for 700 ms. The fixation cross was then replaced by the first prime for 133 ms, which was followed by the second prime for 133 ms. The second prime was then replaced by a blank screen for 34 ms, followed by the target word. Participants were asked to indicate as quickly as possible whether the target word depicts a positive or negative object (evaluative decision task) using a right-hand key (*5* of the number pad) to indicate a positive response, and a left-hand key (*A*) for a negative response. They were also instructed to try not to be distracted by the primes.

Results

Before we tested our hypotheses, we discarded all latencies stemming from anticipations (RT < 300 ms; 0.2%) and incorrect responses (5.5%). Following recommendations by Ratcliff (1993), all of the subsequent analyses were conducted twice: once with a predetermined cutoff-value (in this case 1000 ms) and once with inverse-transformed latencies. The two data sets revealed corresponding patterns of results. For ease of interpretation, we report data with a cutoff of 1000 ms.[2]

To test the influence of evaluative context stimuli in the BFP, we first recoded the manipulation of first prime valence to reflect its evaluative (in)consistency with the valence of the second prime (see Gawronski et al., 2005b). Response latencies were then submitted to a 2 (Second Prime Valence: positive vs. negative) × 2 (First Prime Valence: consistent vs. inconsistent with second prime valence) × 2 (Target Valence: positive vs. negative) ANOVA for repeated measures (see Table 1). This analysis revealed a significant main effect of target valence, $F(1, 52) = 14.00$, $p < .001$, $\eta^2 = .212$, indicating that responses to positive target words ($M = 616.00$, $SE = 6.99$) were generally faster than responses to negative target words ($M = 633.52$, $SE = 6.47$). This main effect was qualified by a significant two-way interaction between second prime valence and target valence, $F(1, 52) = 10.91$, $p = .002$, $\eta^2 = .173$, reflecting the standard affective priming effect. Specifically, participants were faster in responding to positive targets when the second prime was positive ($M = 610.70$, $SE = 7.68$) than when it was negative ($M = 621.27$, $SE = 7.39$). In contrast, participants were faster in responding to negative targets when the second prime was negative ($M = 627.21$, $SE = 7.21$) than when it was positive ($M = 639.82$, $SE = 6.46$). More important to the present question, this two-way interaction was further qualified by a significant three-way interaction between first prime valence, second prime valence, and target valence, $F(1, 52) = 6.46$, $p = .01$, $\eta^2 = .110$. To specify this interaction in terms of the present hypotheses, we conducted separate 2 (Second Prime Valence) × 2 (Target Valence) ANOVAs for the two context conditions.

For evaluatively *inconsistent* context primes, analyses revealed a significant main effect of target valence, $F(1, 52) = 8.29$, $p = .006$, $\eta^2 = .138$, indicating that responses to positive target words ($M = 617.45$, $SE = 7.47$) were faster than responses to negative target words ($M = 633.49$, $SE = 6.55$). More important, the analysis revealed a significant two-way interaction reflecting the standard affective priming effect, $F(1, 52) = 16.15$ $p < .001$, $\eta^2 = .237$. Specifically, participants tended to be faster in responding to positive targets when the second prime was positive than when it was negative, $F(1, 52) = 4.13$, $p = .047$, $\eta^2 = .074$. In contrast, participants were significantly faster in responding to negative targets when the second prime was negative than when it was positive, $F(1, 52) = 14.51$, $p < .001$, $\eta^2 = .218$. The same analysis for evaluatively *consistent* context primes only revealed a significant main effect of target valence, $F(1, 52) = 10.83$, $p = .002$, $\eta^2 = .173$, indicating that responses to positive target words ($M = 614.52$, $SE = 7.48$) were faster than responses to negative target words ($M = 633.54$, $SE = 6.91$). The two-way interaction that would indicate the standard affective priming effect was far from statistical significance, $F(1, 52) = 0.87$, $p = .36$, $\eta^2 = .016$.

To facilitate subsequent comparisons with AMP scores in Experiment 2, we also computed a priming-index, reflecting the relative advantage of positive over negative responses given a particular prime set (sometimes interpreted as an index of automatic positivity). This index was calculated by subtracting the latencies of responses to positive targets from the latencies of responses to negative targets given a particular combination of first and second prime (e.g., Gawronski et al., 2005b).[3] Mean values of the priming index are depicted in Figure 2. Post-hoc comparisons revealed a statistically significant effect of second prime valence in evaluatively inconsistent contexts, $F(1, 52) = 16.15$, $p < .001$, $\eta^2 = .237$, such that positive second primes resulted in a stronger advantage of positive responses compared to negative second primes. This contrast is statistically equivalent to the two-way interaction of second prime valence and target valence with inconsistent first primes (see above). In evaluatively consistent contexts, however, the main effect of second prime valence failed to reach significance, $F(1, 52) = 0.87$, p = .36, $\eta^2 = .016$. This contrast is statistically equivalent to the non-significant two-way interaction of second prime valence and target valence with consistent first primes (see above).

In addition to affective priming effects, we also tested for non-evaluative priming effects of the two semantic categories implied by the employed stimuli (i.e., animate vs. inanimate). For this purpose, we first recoded the category of the first prime to reflect it's (in)consistency with the category of the second prime. Response latencies were then submitted to a 2 (Second Prime Category: animate vs. inanimate) × 2 (First Prime Category: consistent vs. inconsistent with second prime category) × 2 (Target Category: animate vs. inanimate) ANOVA for repeated measures. This analysis revealed a significant main effect of the target category, indicating that participants were faster in responding to target words depicting inanimate objects ($M = 617.44$, $SE = 6.90$) as compared to target words depicting animate objects ($M = 631.73$, $SE = 6.35$), $F(1, 52) = 14.56$, $p < .001$, $\eta^2 = .219$. No other main or interaction reached statistical significance.

Discussion

Results from Experiment 1 indicate that affective priming effects were stronger when a given prime stimulus was preceded by an evaluative incongruent context prime than when it was preceded by an evaluatively congruent context prime. At first glance, these results may be interpreted as reflecting a general principle of automatic evaluations. Specifically, one could argue that affective responses are not determined by the absolute hedonic level of a given event or stimulus, but by the direction and size of *change* in the hedonic level (Brickman et al., 1978). As such, affective reactions to a given stimulus should be enhanced in a context of the opposite valence, but reduced in the context of the same valence. Alternatively, however, these results could also reflect method-related characteristics of the employed measure. Consistent with the finding that RI effects in Fazio et al.'s (1995) BFP depend on participants' attention to the valence of the primes (e.g., Musch & Klauer, 2001; Simmons & Prentice, 2006; Spruyt et al., 2007), evaluatively inconsistent context stimuli may increase the salience of the valence of a given prime (see Cacioppo et al., 1993; Gawronski et al., 2005c), and hence the response tendency elicited by that prime. From this perspective, the obtained contrast effects may stem from a mechanism that is specific to the measure, namely attentional processes involved in RI tasks. Experiment 2 was designed to disentangle these two interpretations. If the obtained contrast effects are driven by construct-related principles of automatic evaluation, the same contrast effects should occur in affective priming measures that do not involve an RI component. If, however, the obtained contrast effects are driven by task-related RI mechanisms, these contrast effects should disappear—or even reverse—in affective priming measures that do not involve an RI component.

Experiment 2

The goal of Experiment 2 was to study affective priming effects resulting from two sequentially presented prime stimuli in an implicit measure that does not contain an RI component, namely Payne et al.'s (2005) AMP. For this purpose, Experiment 2 used exactly the same priming stimuli and presentation times as in Experiment 1. However, Experiment 2 differed from Experiment 1 with respect to the focal task. Specifically, participants were presented with a neutral Chinese character immediately after the presentation of the two primes and their task was to indicate whether they perceived the Chinese character to be more pleasant or less pleasant than average. If the contextual contrast effect observed in Experiment 1 was due to context-induced changes in stimulus valence, a similar effect should be obtained with the AMP. If, however, contrast effects were driven by the operation of method-specific RI processes, these contrast effects should disappear in the AMP. To the contrary, given that misattribution processes are capable of integrating independent sources of affect (e.g., Murphy et al., 1995), the two sequentially presented primes may even result in additive (rather than contrastive) effects in the AMP.

Method

Participants and design. Forty University of Western Ontario undergraduates (30 female, 10 male) participated in Experiment 2. All subjects received course credit for their participation. The experiment consisted of a 2 (First Prime Valence: positive vs. negative) × 2 (First Prime Category: animate vs. inanimate) × 2 (Second Prime Valence: positive vs. negative) × 2 (Second Prime Category: animate vs. inanimate) within-subjects design. Due to a computer malfunction, data from one participant were only partially recorded, and were thus excluded from analyses.

Materials and procedure. Prime stimuli were identical to those used in Experiment 1 (see Appendix). Two of the three subsets were randomly selected as first primes and second primes in the AMP. As target stimuli, we used 128 Chinese characters adapted from Payne et al. (2005). The procedure of the priming task was identical to Experiment 1, except for the presentation of the target stimuli and the required responses to these stimuli. Each trial began with the presentation of a blank screen for 700 ms, followed by a fixation cross (+) for 700 ms. The fixation cross was then replaced by the first prime for 133 ms, which was followed by the second prime for 133 ms. The second prime was then replaced by a blank screen for 34 ms, after which a neutral Chinese character was displayed for 100 ms. The Chinese character, was then replaced by a black-and-white pattern mask and participants had to indicate whether they considered the Chinese character to be more pleasant or less pleasant than the average Chinese character. Following the instructions employed by Payne et al. (2005), participants were told that the words can sometimes bias people's responses to the Chinese characters, and that they should try their absolute best not to let the words bias their judgments of the Chinese characters.

Results

To test the impact of evaluative context stimuli in the AMP, we first calculated the mean proportion of pleasant responses for each of the four prime combinations (i.e., positive-positive, negative-positive, positive-negative, negative-negative). Thus, higher values indicate a higher level of positivity in response to a given prime combination. Following the data analytic procedure employed in Experiment 1, we then recoded the manipulation of the first prime valence to reflect its evaluative (in)consistency with the valence of the second prime (see Gawronski et al., 2005b). The mean proportions of more pleasant responses were submitted to a 2 (Second Prime Valence: positive vs. negative) × 2 (First Prime Valence: consistent vs. inconsistent with second prime valence) ANOVA for repeated measures. This analysis revealed a main effect of the second prime, $F(1, 39) = 27.03$, $p < .001$, $\eta^2 = .409$, indicating a higher proportion of more pleasant responses when the second prime was positive ($M = .588$, $SE = .017$) than when it was negative ($M = .442$, $SE = .030$). This main effect was qualified by a significant two-way interaction between first prime valence and second prime valence, $F(1, 39) = 5.87$, $p = .02$, $\eta^2 = .131$ (see Figure 3), indicating that the affective priming effect of the second prime was more pronounced when the first prime was evaluatively consistent with the second prime than when the first prime was evaluatively inconsistent with the second prime. More precisely, when the first prime was evaluatively consistent with the second prime, participants showed a significantly higher proportion of more pleasant responses when the second prime was positive than when it was negative, $F(1, 39) = 21.82$, $p < .001$, $\eta^2 = .359$. However, this effect was weaker, albeit still significant, when the first prime was inconsistent with the second prime, $F(1, 39) = 15.07$, $p < .001$, $\eta^2 = .279$.

In addition to affective priming effects, we also tested for non-evaluative priming effects of the two semantic categories implied by the employed stimuli (i.e., animate vs. inanimate). For this purpose, we recoded the category of the first prime to reflect its semantic (in)consistency with the category of the second prime. The proportions of more pleasant responses were then submitted to a 2 (Second Prime Category: animate vs. inanimate) × 2 (First Prime Category: consistent vs. inconsistent with second prime category) ANOVA for repeated measures. No main or interaction effect reached statistical significance.

Discussion

The results of Experiment 2 support the notion that experimentally induced variations in implicit measures can sometimes reflect task-specific rather than construct-specific effects. Although the prime and context stimuli were identical to those of Experiment 1, Experiment 2 revealed a pattern of results that is in direct opposition to the one obtained in Experiment 1. Specifically, affective priming effects in Experiment 2 were stronger when the primes were presented in evaluatively congruent contexts than when they were presented in evaluatively incongruent contexts. In other words, the valence of the context and the valence of the prime had an additive effect on affective priming scores in Payne et al.'s (2005) AMP. These additive effects stand in contrast to the results of Experiment 1, in which we obtained contrast effects for Fazio et al.'s (1995) BFP. In that study, stronger priming effects were observed for evaluatively incongruent than evaluatively congruent contexts. Based on the aforementioned differences between the BFP and the AMP, we argue that the obtained dissociation has their roots in the presence versus absence of RI mechanisms. Evaluatively inconsistent primes presumably increase the salience of the valence of the second prime, and hence, prime-related response tendencies in the BFP. In contrast, the affect aroused by the primes that is misattributed to the targets in the AMP presumably follows an additive function (Murphy et al., 1995), resulting in accentuated affect for evaluatively consistent primes and reduced affect for evaluatively inconsistent primes.

Experiment 3

Even though the results of Experiments 1 and 2 are consistent with our interpretation, one could still object that the contrast effect in the BFP may reflect a genuine characteristic of automatic evaluations that cannot be captured by the misattribution mechanism of the AMP. From this perspective, the obtained dissociation has to be attributed to a distorting effect on the task-specific mediator in the AMP (i.e., misattribution) rather than the BFP (i.e., response interference). To rule out this concern, Experiment 3 tested whether two sequential primes produce additive or contrastive effects under conditions of semantic instead of affective double priming. This question is based on previous research, showing additive effects in line with the principles of spreading activation (Collins & Loftus, 1975) in a semantic priming task that used two sequential primes and a setup that does not involve RI (Balota & Paul, 1996). Hence, if the contrast effects observed in Experiment 1 were caused by construct-related features of affective processing rather than method-related factors pertaining to RI, they should turn into additive effects in a non-evaluative, semantic priming variant of the BFP. If, on the other hand, RI was the primary cause of contrast in Experiment 1, a non-evaluative, semantic variant of the BFP should produce the same contrast effects obtained in Experiment 1, despite earlier evidence for additive effects of two sequential primes in semantic priming tasks that do not involve RI (e.g., Balota & Paul, 1996).

To test these alternatives, Experiment 3 used the same prime stimuli that were employed in Experiment 1, which varied in terms of evaluative (i.e., positive vs. negative) and semantic (i.e., animate vs. inanimate) categories. In the present study, these stimuli were used in a semantic priming task similar to the BFP employed in Experiment 1, the only difference being that participants were now instructed to categorize the target words as depicting animate or inanimate objects rather than in terms of their valence (see De Houwer et al., 2002). Following the RI logic outlined for the BFP, we expected that a context prime of the opposite semantic category should increase the salience of the semantic category of the second prime, thereby increasing RI-related priming effects. Conversely, a context prime of the same semantic category should reduce the salience of the semantic category of the second prime, thereby reducing RI-related priming effects.

Method

Participants and design. Eighty-eight University of Western Ontario undergraduates (49 female, 39 male) participated in Experiment 3, receiving course credit for their participation. The experiment consisted of a 2 (First Prime Valence: positive vs. negative) × 2 (First Prime Category: animate vs. inanimate) × 2 (Second Prime Valence: positive vs. negative) × 2 (Second Prime Category: animate vs. inanimate) × 2 (Target Valence: positive vs. negative) × 2 (Target Category: animate vs. inanimate) within-subjects design.

Materials and procedure. As prime stimuli, we used the same 15 words that have been used in Experiments 1 and 2 (see Appendix). Each set of word stimuli was divided into the same three subsets, resulting in three sets of five stimuli for each of the four stimulus categories (i.e., positive animate, negative animate, positive inanimate, negative inanimate). The particular position of each subset (i.e., first prime, second prime, target) was counterbalanced across participants. The procedure of the modified BFP in Experiment 3 was identical to the one in Experiment 1, the only exception being that participants in Experiment 1 made evaluative decisions about the target stimuli, whereas participants in Experiment 3 categorized target words as either representing an animate or inanimate object. As with Experiments 1 and 2, each trial began with the presentation of a blank screen for 700 ms, followed by a fixation cross (+) for 700 ms. The fixation cross was then replaced by the first prime for 133 ms, which was followed by the second prime for 133 ms. The second prime was then replaced by a blank screen for 34 ms, followed by the target stimulus. Participants were asked to indicate as quickly as possible whether the target word depicts an animate or an inanimate object (semantic decision task), using a right-hand key (*5* of the number pad) for animate responses, and a left-hand key (*A*) for inanimate responses. They were also instructed to try not to be distracted by the primes.

Results

The data of Experiment 3 were aggregated according to the procedures described for Experiment 1. We discarded all latencies stemming from anticipations (RT < 300 ms; 0.6%) and incorrect responses (5.0 %). Following recommendations by Ratcliff (1993), all of the subsequent analyses were conducted twice: once with a predetermined cutoff-value (in this case 1000 ms) and once with inverse-transformed latencies. The two data sets revealed corresponding patterns of results. For ease of interpretation, we report data with a cutoff of 1000 ms.

To test the influence of semantic context stimuli in the BFP variant using a semantic decision task, we first recoded the category of the first prime to reflect its (in)consistency with the category of the second prime (see Gawronski et al., 2005b). Response latencies were then submitted to a 2 (Second Prime Category: animate vs. inanimate) × 2 (First Prime Category: consistent vs. inconsistent with second prime category) × 2 (Target Category: animate vs. inanimate) ANOVA for repeated measures. This analysis revealed a significant main effect of the target category, indicating that responses were faster to target words depicting animate objects (M = 625.46, SE = 5.74) as compared to target words depicting inanimate objects (M = 651.13, SE = 6.05), $F(1, 87) = 54.12$, $p < .001$, $\eta^2 = .383$. This main effect was qualified by a significant two-way interaction between second prime category and target category, $F(1, 87) = 24.22$, $p < .001$, $\eta^2 = .218$, indicating a semantic priming effect of the second prime. Specifically, participants were faster in responding to animate targets when the second prime depicted an animate object (M = 619.73, SE = 6.03) than when it depicted an inanimate object (M = 631.19, SE = 5.79). In contrast, participants were faster in responding to inanimate targets when the second prime depicted an inanimate object (M = 646.21, SE = 6.57) than when it depicted an animate object (M = 656.05, SE = 5.90). More important to the present question, this interaction was qualified by a significant three-way interaction between first prime category, second prime category, and target category, $F(1, 87) = 18.16$, $p < .001$, $\eta^2 = .173$ (see Table 2). To specify this interaction in terms of the present hypotheses, we conducted separate 2 (Second Prime Category) × 2 (Target Category) ANOVAs for the two context conditions.

For semantically *inconsistent* context primes, analyses revealed a significant main effect of target category, $F(1, 87) = 45.12$, $p < .001$, $\eta^2 = .341$, indicating that responses were faster to target words depicting animate objects (M = 627.48, SE = 6.21) as compared to target words depicting inanimate objects (M = 652.12, SE = 6.07). More important, the analysis revealed a significant two-way interaction of second prime category and target category, $F(1, 87) = 36.97$, $p < .001$, $\eta^2 = .298$. Specifically, participants were faster in responding to animate objects when the second prime depicted an animate object than when it depicted an inanimate object, $F(1, 87) = 27.31$, $p < .001$, $\eta^2 = .239$. Conversely, participants were faster in responding to inanimate objects when the second prime depicted an inanimate object than when it depicted an animate object, $F(1, 87) = 14.00$, $p < .001$, $\eta^2 = .139$. The same analysis for evaluatively *consistent* context primes revealed only a significant main effect of the target category, $F(1, 87) = 31.85$, $p < .001$, $\eta^2 = .268$, again indicating that responses were faster to target words depicting animate objects ($M = 623.44$, $SE = 5.80$) as compared to target words depicting inanimate objects ($M = 650.14$, $SE = 6.39$). The two-way interaction that would reflect a semantic priming effect of the second prime was far from statistical significance, $F(1, 87) = 0.27$, $p = .61$, $\eta^2 = .003$.

To facilitate comparisons with corresponding AMP scores in Experiment 4, we also calculated a priming index, reflecting the relative advantage of responding to words depicting animate objects over words depicting inanimate objects given a particular prime set. This index was calculated by subtracting the latencies of responses to animate targets from the latencies of responses to animate targets given a particular combination of first and second primes. Mean values of the priming index are printed in Figure 4. Replicating the pattern obtained for the BFP in Experiment 1, post-hoc comparisons revealed that the effect of the second prime category was significant in the context of semantically inconsistent first primes, $F(1, 87) = 36.97$, $p < .001$, $\eta^2 = .298$. This contrast is statistically equivalent to the two-way interaction of second prime category and target category with inconsistent first primes (see above). However, in the context of semantically consistent first primes, the effect of the second prime category was far from statistical significance, $F(1, 87) = 0.27$, $p = .606$, $\eta^2 = .003$. This contrast is statistically equivalent to the non-significant two-way interaction of second prime category and target category with consistent first primes (see above).

Corresponding to the analyses for Experiment 1, we also tested for affective priming effects resulting from the valence implied by the employed stimuli. For this purpose, we first recoded the valence of the first prime to reflect it’s (in)consistency with the valence of the second prime. Response latencies were then submitted to a 2 (Second Prime Valence: positive vs. negative) × 2 (First Prime Valence: consistent vs. inconsistent with second prime valence) × 2 (Target Category: positive vs. negative) ANOVA for repeated measures. This analysis revealed a significant main effect of target valence, indicating that participants were faster in responding to positive (M = 634.31, SE = 5.80) as compared to negative target words (M = 641.82, SE = 5.68), $F(1, 87) = 10.03$, $p = .002$, $\eta^2 = .103$. No other main or interaction reached statistical significance.

Discussion

Results from Experiment 3 further corroborate our assumption that experimental effects on implicit measures may sometimes reflect changes in the task-specific mediator rather than automatic evaluations. In the present study, we replicated the contextual contrast effects observed in Experiment 1 with a semantic variant of Fazio et al.'s (1995) BFP. This result indicates that contextual contrast effects in the BFP are not specific to automatic evaluations. Instead, such contrast effects generalize to non-evaluative variants of the same priming paradigm. In addition, the results of Experiments 1 and 3 provide further support for the previously obtained goal-dependency of priming effects in the BFP (e.g., De Houwer et al., 2002; Klauer & Musch, 2002; Klinger et al., 2000). Particularly, we observed affective priming effects only when participants had the goal of categorizing the targets according to their valence (Experiment 1), but not when their goal was to categorize the targets in terms of a non-evaluative, semantic category (Experiment 3). Likewise, we observed semantic priming effects only when participants had the goal of categorizing the targets in terms of the very semantic category (Experiment 3), but not when their goal was to categorize them according to their valence (Experiment 1). These results support the conclusion that priming effects in the BFP are primarily due to late, response-related processes, presumably tied to the preparation of a response to the target stimulus (De Houwer et al., 2002; Klauer et al., 2005; Spruyt et al., 2007). At the same time, the present findings suggest that passive processes of spreading activation from primes to targets may play a less significant role, given that in the present set-up spreading activation should result in additive rather than contrastive effects of two sequentially presented primes (e.g., Balota & Paul, 1996).

Experiment 4

Even though the results of Experiment 3 are consistent with our assumption that contrast effects in the BFP are driven by method-related factors pertaining to RI, the alternative outcome of additive effects resulting from construct-related processes of spreading activation (Collins & Loftus, 1975) was based on research that used setups and materials that strongly deviated from the ones employed in the present studies (Balota & Paul, 1996). Thus, it seems important to replicate the additive effects obtained in earlier research for the current setup and materials. To address this concern, Experiment 4 tested whether the additive effects obtained for the AMP also generalize to non-affective materials. Paralleling our reasoning in Experiment 3, we hypothesized that the misattribution mechanism underlying the AMP is not specific to affect or evaluation. This assumption is derived from earlier research, showing that misattribution effects can also occur with non-evaluative qualities, such as cognitive feelings (e.g., Strack & Neumann, 2000). To test this hypothesis, participants in Experiment 4 completed a task similar to the AMP employed in Experiment 2, using the same prime stimuli that were employed in the previous experiments. However, instead of judging the visual pleasantness of the Chinese characters, participants were asked to guess whether the Chinese character refers to an animate or an inanimate object. Based on the findings obtained in Experiment 2, we expected that two sequentially presented prime stimuli would influence guessing responses in an additive manner, such that two animate (inanimate) primes should result in more animate (inanimate) interpretations of the Chinese characters than a combination of two semantically inconsistent primes.

Method

Participants and design. Thirty-eight University of Western Ontario undergraduates (27 female, 11 male) participated in Experiment 4. All subjects received course credit for their participation. Experiment 4 consisted of a 2 (First Prime Valence: positive vs. negative) × 2 (First Prime Category: animate vs. inanimate) × 2 (Second Prime Valence: positive vs. negative) × 2 (Second Prime Category: animate vs. inanimate) within-subjects design.

Materials and procedure. Prime and target stimuli were identical to Experiment 2. The procedure of the modified AMP in Experiment 4 was also identical to the one in Experiment 2, the only exception being that participants in Experiment 4 were asked to guess whether the Chinese character depicts an animate or an inanimate object, using a right-hand key (*5* of the number pad) for *animate*, and a left-hand key (*A*) for *inanimate*. As with Experiment 2, participants were told that the words can sometimes bias people's responses to the Chinese characters, and that they should try their absolute best not to let the words bias their judgments of the Chinese characters (see Payne et al., 2005).

Results

Parallel to Experiment 2, we calculated the mean proportion of animate responses for each of the four prime combinations, with higher values indicating a higher level of animate guesses in response to a given prime combination. To test the influence of semantic context stimuli in the AMP variant using a semantic guessing task, we recoded the category of the first prime to reflect its semantic (in)consistency with the category of the second prime (see

Gawronski et al., 2005b). The mean proportions of animate responses were then submitted to a 2 (Second Prime Category: animate vs. inanimate) × 2 (First Prime Category: consistent vs. inconsistent with second prime category) ANOVA for repeated measures, indicating a higher proportion of animate responses when the second prime word depicted an animate object (M = .570, SE = .026) than when it depicted an inanimate object (M = .393, SE = .022), $F(1, 37) = 26.72$, $p < .001$, $\eta^2 = .419$. This main effect was qualified by a significant two-way interaction between first and second prime, $F(1, 37) = 14.47$, $p = .001$, $\eta^2 = .281$ (see Figure 5). Consistent with the present predictions, the semantic priming effect of the second prime was stronger when the first prime was semantically consistent with the second prime than when the first prime was semantically inconsistent with the second prime. More precisely, when the first prime was semantically consistent with the second prime, participants showed a significantly higher proportion of animate responses when the second prime word depicted an animate object than when it depicted an inanimate object, $F(1, 37) = 28.16$, $p < .001$, $\eta^2 = .432$. However, this effect was much weaker, though still significant, when the first prime was inconsistent with the second prime, $F(1, 37) = 9.08$, $p = .005$, $\eta^2 = .197$.

In addition to goal-relevant priming effects of the animate-inanimate dimension, we also tested for goal-irrelevant priming effects of the valence of the employed stimuli. For this purpose, we recoded the valence of the first prime to reflect its evaluative (in)consistency with the valence of the second prime. The mean proportions of animate responses were then submitted to a 2 (Second Prime Valence: positive vs. negative) × 2 (First Prime Valence: consistent vs. inconsistent with second prime valence) ANOVA for repeated measures. Somewhat to our surprise, this ANOVA revealed a significant main effect of second prime valence, such that participants were more likely to guess animate when the second prime was positive (M = .517, SE = .020) than when it was negative (M = .446, SE = .021), $F(1, 37)$ = 11.48, p = .002, η^2 = .237. This main effect was qualified by a significant two-way interaction of first prime valence and second prime valence, indicating that this effect was statistically significant only when the first prime was evaluatively consistent with the second prime ($M_{positive}$ = .554, $SE_{positive}$ = .028 vs. $M_{negative}$ = .427, $SE_{negative}$ = .025), $F(1, 37)$ = 11.81, p = .001, η^2 = .242, but not when it was evaluatively inconsistent with the second prime ($M_{positive}$ = .479, $SE_{positive}$ = .024 vs. $M_{negative}$ = .465, $SE_{negative}$ = .020), $F(1, 37)$ = 0.49, p = .49, η^2 = .013.

Discussion

In combination with Experiment 3, the results from Experiment 4 further highlight the difference between priming tasks that do versus do not involve an RI component. In Experiment 4, we replicated the previously obtained additive context effects for a semantic variant of Payne et al.'s (2005) AMP. This result stands in contrast to the findings of Experiment 3, which demonstrated contextual contrast effects for a semantic variant of Fazio et al.'s (1995) BFP. Moreover, the present findings indicate that the two kinds of context effects are not specific to measures of automatic evaluation. Instead, these context effects generalized to non-evaluative variants of the employed measures, providing further support for our assumption that experimentally induced changes may sometimes be driven by method-related rather than construct-related factors.

Somewhat to our surprise, Experiment 4 also found a priming effect of prime valence on animate-inanimate guessing. Specifically, participants were more likely to guess animate when the second prime was positive than when it was negative. This effect was particularly pronounced in the context of evaluatively consistent stimuli, but disappeared for evaluatively inconsistent context stimuli. A possible interpretation for this unexpected finding is that positive primes elicited a positive affective reaction, which then resulted in a general tendency to provide an affirmative response. Given that animate guesses resemble an affirmation response and inanimate guesses resemble a negation response, the valence of the primes could systematically influence non-evaluative guessing processes.[4] This finding may indicate a potential problem with applying Payne et al.'s (2005) paradigm to non-evaluative dimensions. If non-evaluative responses to the neutral Chinese characters can be influenced by judgment-irrelevant features of the primes, the resulting priming scores could be systematically contaminated by contingent features of the employed prime stimuli. Future research should further investigate the range and potential limits of Payne et al.'s (2005) paradigm for non-evaluative dimensions.

General Discussion

The main goal of the present research was to show that experimentally induced variability in implicit measures may sometimes reflect secondary changes driven by task-specific mediators rather than genuine changes in automatic evaluations. Using double-priming effects as an example (Gawronski et al., 2005b), the present studies indicate that the same manipulation can even lead to opposite effects on otherwise similar measures, when these measures differ with regard to their underlying mechanisms. Across four studies, we found that multiple primes resulted in contrast effects in evaluative (Experiment 1) and semantic (Experiment 3) variants of Fazio et al.'s (1995) BFP. However, the same manipulation led to *additive effects i*n evaluative (Experiment 2) and semantic (Experiment 4) variants of Payne et al.'s (2005) AMP. Drawing on earlier studies showing attentional influences on RI tasks (e.g., Besner & Stolz, 1999; Besner et al., 1997; Musch & Klauer, 2001; Simmons & Prentice, 2006; Spruyt et al., 2007; see also Proctor & Cho, 2006), we argue that these differences are driven by the operation of RI processes in the BFP, which are not present in the AMP. Specifically, we proposed that the relative size of RI effects in the BFP depends on participants' attention to the relevant feature of the prime (e.g., valence). To the degree that the salience of a given prime feature is increased in the context of a stimulus of the opposite feature (e.g., Cacioppo et al., 1993), such context stimuli may enhance RI effects in the BFP via secondary attentional processes. This situation is different in the AMP, which has recently been shown to be immune against attentional influences (Gawronski, Cunningham, LeBel, & Deutsch, 2008b). Consistent with this claim, AMP scores in the present studies reflected additive context effects, as they are predicted by spreading activation models (Collins & Loftus, 1975) and as they have been shown in earlier research (e.g., Balota & Paul, 1996). Thus, interpreting contrast effects on BFP scores as reflecting genuine changes in automatic evaluations would have the potential to seriously distort theorizing about the nature of automatic evaluations.

Understanding Contrast Effects in Sequential Priming

Notwithstanding our interpretation of contextual contrast effects in terms of attentional accentuation, contrast effects in perception and judgment may arise from at least three other mechanisms, which can be described as (a) perceptual contrast, (b) correction contrast, and (c) comparison contrast (for reviews, see Suls & Wheeler, 2007; Wedell, Hicklin & Smarandescu, 2007).

According to the notion of perceptual contrast, the basic experience of a perceptual event is often biased in the direction opposite to the experiences that occur in temporal or spatial proximity (Wedell et al., 2007). For example, lukewarm water typically appears hot after having placed one's hand in ice water, whereas the same water appears cold after holding one's hand in hot water. Thus, in line with this emphasis on basic experiences, one could argue that the contrast effects obtained in the present studies resemble the notion of perceptual contrast, in that automatic evaluations may be determined by the direction and size of change in hedonic experiences (Brickman et al., 1978), rather than by the absolute hedonic level of a given event or stimulus. However, in evaluating this interpretation, it is important to note that this mechanism predicts a genuine change in automatic evaluations (see Figure 1), and hence corresponding effects for the BFP and the AMP. This prediction stands in contrast to the present findings showing contrast effects for the BFP, but additive effects for the AMP. As such, perceptual contrast does not seem to represent a viable account for the present results.

The second possible mechanism, correction contrast, implies that people try to correct their judgments for potentially biasing influences. For example, when evaluating the intellectual ability of a highly attractive person, evaluators may adjust their subjective assessment if they suspect that attractiveness may bias judgments of intelligence. Crucial for the present discussion, recent research suggests that such correction processes may be highly efficient, leading to correction for unwanted influences even in implicit measurement procedures such as priming paradigms (e.g., Glaser, 2007; Glaser & Banaji, 1999; Maier, Berner, Hau, & Pekrun, 2007). Could the contrast effects observed in the present experiments reflect participants' efforts not to be influenced by the first primes? At first sight, this seems plausible given that participants in the BFP were potentially aware of a biasing influence, and given that the BFP, but not the AMP, emphasizes accurate responding. Both conditions have been shown to promote correction contrast in implicit measures (Glaser & Banaji, 1999; Maier et al., 2007; Maier, Berner, & Pekrun, 2003). Note, however, that this account fails to explain why participants did not correct for the biasing influence of the second prime in the BFP. It also cannot explain why studies that used only one instead of two primes failed to observe contrast effects for the same SOA of 300 ms (e.g., Fazio et al., 1986; Hermans, Spruyt, & Eelen, 2003). Based on these considerations, correction contrast does not seem to provide a viable explanation for the present findings.

The third mechanism, comparison contrast, operates when a stimulus is used as a standard of comparison for another stimulus (e.g., Mussweiler, 2003; Stapel, 2007). For example, a common criminal may be judged as being less evil when this person is compared to Adolf Hitler than when this criminal is compared to Mahatma Gandhi. In line with this reasoning, it seems possible that participants used the first prime as a standard of comparison for the second prime, which should accentuate the valence of the second prime if the first prime was evaluatively incongruent. However, as with the proposed explanation in terms of perceptual contrast, this account raises the question of why such comparison processes do not operate in the AMP, where two sequential primes showed additive effects. Hence, to maintain this alternative explanation in the light of the obtained dissociation, one would still have to draw on procedural differences between the two tasks. For example, based on Stapel's (e.g., Stapel, 2007; Stapel & Koomen, 2001) interpretation-comparison model one could argue that the evaluatively unambiguous targets in the BFP generally induce a comparative mindset, which has been shown to promote contrast effects. Conversely, the evaluatively ambiguous targets in the AMP may induce an interpretative mindset, thereby favoring assimilation. Note, however, that while this assumption bears some plausibility, one would also have to make the implausible and less parsimonious assumption that a comparative mindset in the BFP selectively triggers comparisons of multiple primes, but not comparisons between primes and targets. Otherwise, this explanation would (falsely) predict contrast effects of single primes presented at comparable SOAs. Thus, even comparison contrast does not seem to provide a viable explanation for the present findings.

Procedural Differences and Task-Specific Mediators

In the present research, we predicted antagonistic effects for the BFP and the AMP based on the assumption that the BFP is primarily driven by RI, whereas the AMP is primarily driven by misattribution. Although the obtained results generally supported our predictions, it is still an open question which particular features of the two measures are ultimately responsible for the obtained dissociation. In the following sections, we discuss this question for the five most apparent procedural differences. Our central claim is that some of these features are essential for the proposed difference between RI and misattribution, and thus for the emergence of additive versus contrastive effects. Yet, other features may simply enhance or reduce basic priming effects driven by a given mediator, which may inherently enhance or reduce the respective type of context effect for each of the two measures. Finally, some features seem irrelevant for the proposed difference between RI and misattribution, and therefore should leave the obtained dissociation between BFP and AMP measures unaffected.

The first difference is that the target stimuli in the AMP are of neutral valence and semantically meaningless for participants, whereas the targets in the BFP are of clear semantic and evaluative meaning. As we have argued in the introduction, the latter is essential for the operation of RI in the BFP, as otherwise there would be no target-related response tendency that could be congruent or incongruent with the response tendency elicited by the prime. At the same time, a lack of semantic and evaluative meaning seems crucial for misattribution to occur, as participants may otherwise base their judgments on response-relevant features of the target (e.g. Mayer & Merckelbach, 1999). As such, our account implies that using neutral target words in an otherwise unchanged BFP should yield the same additive effects that have been obtained for the

AMP. Conversely, using evaluatively meaningful targets in an otherwise unchanged AMP should result in the same contrast effects that have been obtained for the BFP.

A second important difference is that participants in the BFP typically work under accuracy instructions, whereas no such instructions are given in the AMP. In fact, such instructions would make little sense in the AMP, as there is no accurate response defined for evaluatively neutral targets. In the BFP, accuracy instructions may undermine people's propensity to use their affective states to categorize the target, which are subjective by definition, and therefore cannot be correct or incorrect. Based on these considerations, it seems possible that omitting accuracy instructions in an otherwise unchanged BFP may promote the emergence misattribution effects by the prime stimuli over and above the impact of RI. As such, contrast effects elicited by RI may be compensated by newly introduced additive effects resulting from misattribution, thereby leading to a reduction of contrast effects in the BFP when accuracy instructions are dropped.

A third difference is that participants in the BFP are required to respond as quickly as possible, whereas no such speed instructions are given in the AMP. In the BFP, speed instructions may be important for the emergence of RI effects, given that the requirement to respond quickly may facilitate the creation of short-term stimulus-response associations. At the same time, speed instructions may promote quick and superficial processing of the neutral targets in the AMP, which may enhance the misattribution of prime characteristics to the targets. Based on these considerations, it seems possible that omitting speed instructions in the BFP may be detrimental to emergence of priming effects based on RI, and thus for contrast effects resulting from RI. To the degree that speed instructions enhance the misattribution of affective states to neutral stimuli, including speed instructions in the AMP may increase basic priming effects in this task, and thereby additive effects of context primes.

A fourth important difference is that the targets in the AMP are presented only briefly and are replaced by a masking stimulus, whereas the targets in the BFP typically remain on the screen until participants have made their decision. Even though we cannot think of any reason why a short and masked presentation of target stimuli may influence priming effects in the BFP, a limited opportunity for target processing may be crucial for misattribution effects in the AMP. Specifically, suboptimal processing conditions limit participants' ability to base their judgments on particular features of the targets, which in turn may enhance their reliance on momentary feelings for evaluating the target stimuli. In other words, short and masked presentations of the target stimuli may not result in any changes in the BFP. However, longer, unmasked presentations may attenuate basic priming effects in the AMP, and thereby additive effects of two sequential primes (Murphy & Zajonc, 1993).

A final difference we consider important is that the AMP is based on the proportion of positive versus negative evaluations of the target stimuli, whereas the BFP is typically based on the latencies of target evaluations. Still, affective priming effects in the BFP can also be manifested in proportions of positive versus negative responses, such as error rates for compatible versus incompatible trials when the task includes a response-window (e.g., Klauer, Rossnagel, & Musch, 1997). Thus, depending on the setup of the task (i.e., with or without response-window) priming effects in the BFP may be reflected either in response proportions or in response latencies. Given that the operation of RI should be unaffected by the inclusion of a response-window (Klinger et al., 2000), we would expect the same pattern of results for the BFP regardless of whether priming scores are derived from response proportions or response latencies. Note, however, that this situation is different for the AMP, where analyzing response latencies as a function of the primes is generally uninformative about the emergence of priming effects.

In summary, the above analysis suggests that several of the five major procedural differences may contribute to the functional differences between the AMP and the BFP, which in turn may influence the emergence of additive versus contrast effects of two sequential primes. One feature seems to set a necessary precondition for one or the other mediator: RI can occur only with clearly valenced targets, whereas misattribution can occur only with ambiguous targets. Over and above these necessary preconditions, three additional features seem to set facilitating conditions for one or the other mediator. First, dropping accuracy instructions from the BFP could introduce misattribution effects over and above RI, which may produce compensatory context effects (i.e., additive and contrastive) resulting from the two mechanisms. Second, speed instructions may be essential for the emergence of RI effects in the BFP and beneficial for misattribution effects in the AMP, which in both cases should increase the respective context effects (i.e., contrastive vs. additive) for each of the two measures. Third, short and masked presentations of the target stimuli may be essential for the misattribution of prime characteristics to the targets. As such, longer, unmasked presentations may reduce basic priming effects in the AMP, and therefore the emergence of additive context effects obtained for this measures. Finally, we could not find a theoretically sound reason why priming scores in the BFP should differ for response latencies and response proportions, which makes this particular feature irrelevant for the obtained contrast effects on BFP scores. In sum, although it is possible that one of these procedural differences is predominantly responsible for the obtained dissociation between the BFP and the AMP, we argue that the most critical feature is the presence versus absence of a clear evaluative meaning of the target stimuli, which represents a precondition for each of the two mechanisms. Still, additional research would be useful to further clarify the individual role of the abovementioned features.

Interpreting Experimental Effects on Implicit Measures

The double-priming paradigm employed in the present research implies a deviation from the well-established procedures of the AMP and the BFP. Clearly, no scientist who is using these measures to assess spontaneous evaluations would introduce such changes. Nevertheless, the double-priming paradigm allows one to study the functional role of an important moderator, namely attention to features of the primes (see also Gawronski et al., 2008b). As this moderator is operating in many typical research settings, our conclusions have important implications for the interpretation of experimentally induced differences in implicit measures. Even though it seems reasonable to assume that many of these effects reflect genuine changes in automatic evaluations (Gawronski & Bodenhausen, 2006), the present findings point to alternative explanations for at least some of these studies.

One example concerns the nature of accessibility effects on implicit measure. Resembling the dissociation obtained in the present studies, Gawronski and Bodenhausen (2005) showed that higher amounts of information intentionally retrieved from memory *increase* scores on implicit measures that do not involve an RI component, but *decrease* corresponding scores on implicit measures that do involve an RI component. Drawing on earlier research on ease-of-retrieval effects (Schwarz et al., 1991; for a review, see Schwarz, Bless, Wänke, & Winkielman, 2003), Gawronski and Bodenhausen (2005) argued that implicit measures involving an RI component are influenced by the experienced ease of retrieving information from memory, which typically increases as a function of the amount of information to be retrieved (Schwarz et al., 1991). In contrast, implicit measures that do not involve an RI component seem to be influenced by the momentary activation level of associations in memory, directly reflecting the overall amount of retrieved information. However, even though these assumptions are consistent with the obtained dissociation, Gawronski and Bodenhausen's (2005) data do not provide any information as to *why* different kinds implicit measures are differentially susceptible to the two kinds of influences. The present results suggest that attentional processes may play a significant role in this regard, such that the experienced ease of retrieving information from memory may influence attention to stimulus features in a manner that is opposite to the overall amount of information activated in memory. Thus, given that attention has been shown to influence implicit measures that involve an RI component (e.g., Gawronski et al., 2005b, 2008b; Musch & Klauer, 2001; Simmons & Prentice, 2006; Spruyt et al., 2007; see also Proctor & Cho, 2006), retrieval-related shifts in attention may influence these measures in a manner that is in direct opposition to retrieval-related effects on measures that do not involve an RI component. Future research employing supplementary measures of attention may help to clarify the role of attentional processes for ease-of-retrieval effects on implicit measures.

Recommendations

It is important to note that our considerations do not generally negate the validity of previously observed effects on implicit measures. However, they do suggest that caution should be taken when drawing inferences regarding changes in automatic evaluations. Given that experimental effects on task-specific mediators can distort theorizing about automatic evaluations if these effects are misinterpreted as reflecting genuine changes in evaluative responses, it is essential to distinguish between method-related and construct-related effects on implicit measures. Based on the present research, we recommend supplementing research that aims at investigating experimental effects on implicit measures with the following components:

First, research studying experimental effects on implicit measures should include a theoretical analysis of the task-specific mechanisms that translate automatic evaluations into task performance in the employed measure. Such an analysis will allow one to generate hypotheses about how a given factor may interact with method-related mechanisms. In the present studies, this analysis included earlier findings on attentional influences in RI tasks (e.g., Besner & Stolz, 1999; Besner et al., 1997; Musch & Klauer, 2001; Simmons & Prentice, 2006; Spruyt et al., 2007; see also Proctor & Cho, 2006) and the integration of multiple sources of affect in misattribution (e.g., Murphy et al., 1995). Of course, such analyses require a sufficient understanding of the task-specific mediators underlying implicit measures, and without such knowledge, it will be difficult to predict whether a given measure works "as intended" or will suffer from method-related distortions. To the degree that research on this question is still scarce (for valuable exceptions, see Brendl et al., 2001; Conrey et al., 2005; De Houwer, 2003b; Klauer & Musch, 2003; Rothermund & Wentura, 2004), the present findings point to the importance of more research in this regard.

Second, it seems desirable to validate a given effect with two implicit measures that are presumably based on different mechanisms. For example, if a given manipulation shows identical effects on the BFP and the AMP—two measures that are based on very different mechanisms—the obtained correspondence would provide strong evidence for the method-independent nature of these effects (e.g., Rydell & Gawronski, 2007). To be sure, many experimental effects have been demonstrated for different kinds of implicit measures. However, to our knowledge, there is only a single study (Gawronski & Bodenhausen, 2005) that compared measures that do versus do not involve a RI mechanism. Interestingly, this study, just as the present ones, found antagonistic effects of the same experimental manipulation (i.e., ease-of-retrieval task; see Schwarz et al., 1991). All other studies comparing context effects on different implicit measures used variants that were primarily based on RI, thereby limiting the diagnosticity of such comparisons (for an overview, see Gawronski & Bodenhausen, 2006). This predominant use of RI-based measures is not particularly surprising, given the small number of possible alternatives. As Gawronski et al. (in press) pointed out, only 3 out of 13 common implicit measures do not involve a notion of RI. Future research comparing experimental effects on measures that do versus do not involve a RI component may help to clarify the precise nature of previously obtained effects.

Finally, as far as RI mechanisms are involved, special consideration should be devoted to attentional mechanisms and feature salience (see Gawronski et al., in press). These factors presumably play a crucial role in RI tasks, and are capable of influencing the intensity (e.g., Simmons & Prentice, 2006) or the direction of priming effects, as obtained in the present studies. Independent tests of feature salience (e.g., Rothermund & Wentura, 2004) could provide useful information in this regard.

Conclusion

Researchers often employ implicit measures as dependent variables to investigate processes of attitude formation and change. In such studies, experimentally induced differences are typically interpreted as reflecting change in automatic evaluations. The main goal of the present research was to show that experimentally induced differences in measurement scores may sometimes be driven by changes in the task-specific mediator underlying a given measure rather than genuine changes in automatic evaluations. In the present studies, such effects were reflected in antagonistic effects of the same experimental manipulation on two functionally equivalent affective priming tasks that are based on distinct mechanisms (Fazio et al., 1995; Payne et al., 2005). As misinterpretations of secondary effects on task-specific mediators have the potential to seriously distort theorizing about attitudes and evaluations, researchers should be cautious in interpreting experimentally induced differences in measurement scores as reflecting genuine changes in the underlying evaluations.

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Footnotes

[1] Note that even though Fazio et al. (1995) used the term *Bona Fide Pipeline* in their original presentation of the task, they have rarely used this label since then (for a recent exception, see Olson & Fazio, 2003). In the present article, we use the shortcut BFP for the sake of simplicity to distinguish Fazio et al.'s (1995) task from Payne et al.'s (2005) Affect Misattribution Procedure (AMP).

[2] The employed cut-off values of 300 and 1000 ms were based on the procedures employed by Gawronski et al. (2008a).

[3] Note that responses to positive target words are typically faster than responses to negative words, thereby promoting scores higher than zero. Thus, the resulting priming scores should *not* be interpreted in an absolute manner, such that scores higher than zero would indicate a positive response and scores lower than zero would indicate a negative response. Instead, priming scores should only be interpreted in a *relative* manner, such that higher scores indicate more positive responses.

[4] Note that the particular key assignment of animate-inanimate guessing was not counterbalanced in the present study. Thus, an alternative interpretation of prime valence effects on semantic guessing is that positivity could be inherently mapped to right-hand responses, such that positive affective reactions elicited by the primes enhance the likelihood of right-hand guessing. Future research may test these interpretations by orthogonally mapping affirmation versus negation responses with left-hand and right-hand responses in semantic guessing tasks.

Appendix:

Word Stimuli Used for Experiments 1-4

	Set A	Set B	Set C
Positive Animate	koala	kitten	puppy
	duckling	panda	dolphin
	butterfly	bunny	deer
	kangaroo	hamster	lamb
	swan	seal	parrot
Positive Inanimate	paradise	humor	harmony
	summer	health	love
	sunrise	cheer	freedom
	relaxation	pleasure	peace
	vacation	heaven	honesty
Negative Animate	cockroach	maggot	ticks
	grub	tarantula	hornet
	germs	spider	leech
	mosquito	locust	scorpion
	snake	blackfly	wasp
Negative Inanimate	disaster	abuse	terror
	sickness	prison	murder
	vomit	poison	evil
	garbage	assault	death
	accident	cancer	bomb

Table 1

Mean Response Latencies in Milliseconds and Standard Errors as a Function of Second Prime Valence (Positive vs. Negative), First Prime Valence (Consistent vs. Inconsistent with First Prime), and Target Valence (Positive vs. Negative), Experiment 1.

		First Prime Consistent with Second Prime		First Prime Inconsistent with Second Prime	
		Second Prime Positive	Second Prime Negative	Second Prime Positive	Second Prime Negative
Positive Target					
	M	612	617	610	625
	SE	8.38	8.08	8.50	8.21
Negative Target					
	M	635	632	645	622
	SE	6.74	8.10	7.07	7.35

Table 2

Mean Response Latencies in Milliseconds and Standard Errors as a Function of Second Prime Category (Animate vs. Inanimate), First Prime Category (Consistent vs. Inconsistent with First Prime), and Target Category (Animate vs. Inanimate), Experiment 3.

		First Prime Consistent with Second Prime		First Prime Inconsistent with Second Prime	
		Second Prime Animate	Second Prime Inanimate	Second Prime Animate	Second Prime Inanimate
Animate Target					
	M	626	628	613	634
	SE	6.67	6.53	6.31	5.95
Inanimate Target					
	M	653	652	659	641
	SE	6.38	6.49	6.30	7.39

Figure Captions

Figure 1. Hypothetical sequence of processes mediating between stimulus-presentation and task performance on an implicit measure of evaluation. Experimental manipulations may influence the measurement outcome via two routes: effects on evaluative responses (A) or effects on the task-specific mediator linking evaluative responses and task performance (B).

Figure 2. Mean priming-index as a function of second prime valence (positive vs. negative) and first prime valence (consistent vs. inconsistent with second prime valence) in a sequential priming paradigm using an evaluative decision task (positive vs. negative), Experiment 1. Higher numbers indicate higher levels of "automatic positivity."

Figure 3. Mean proportion of "more pleasant" responses to neutral Chinese characters in the Affect Misattribution Procedure (AMP) as a function of second prime valence (positive vs. negative) and first prime valence (consistent vs. inconsistent with second prime valence), Experiment 2. Higher numbers indicate higher levels of "automatic positivity."

Figure 4. Mean priming-index as a function of second prime category (animate vs. inanimate) and first prime category (consistent vs. inconsistent with second prime category) in a sequential priming paradigm using a semantic decision task (animate vs. inanimate), Experiment 3. Higher numbers indicate higher levels "automatic activation" of the concept animate.

Figure 5. Mean proportion of "animate" responses to neutral Chinese characters in the Affect Misattribution Procedure (AMP) as a function of second prime category (animate vs. inanimate) and first prime category (consistent vs. inconsistent with second prime category), Experiment 4. Higher numbers indicate higher levels "automatic activation" of the concept animate.

Figure1

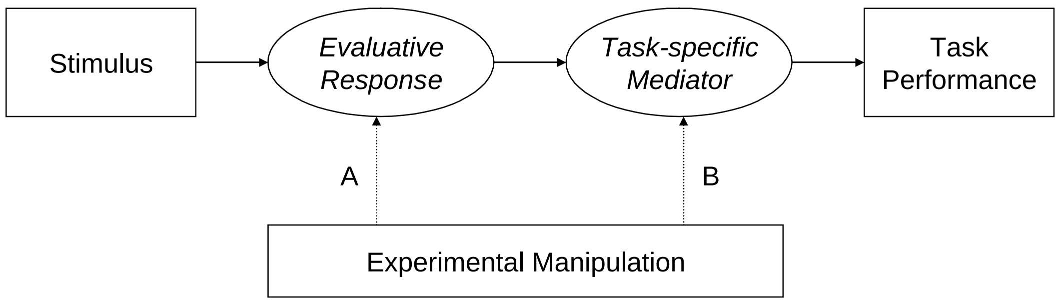

Stimulus
Evaluative Response
Task-specific Mediator
Task Performance
A
B
Experimental Manipulation

Figure2

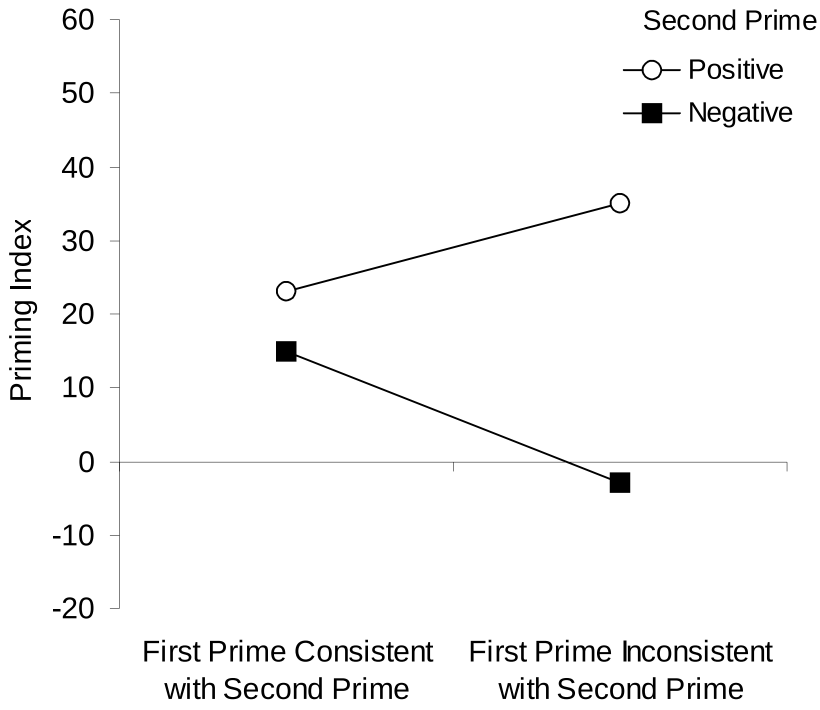

60
50
40
30
20
10
0
-10
-20
Priming Index
Second Prime
Positive
Negative
First Prime Consistent with Second Prime
First Prime Inconsistent with Second Prime

Figure3

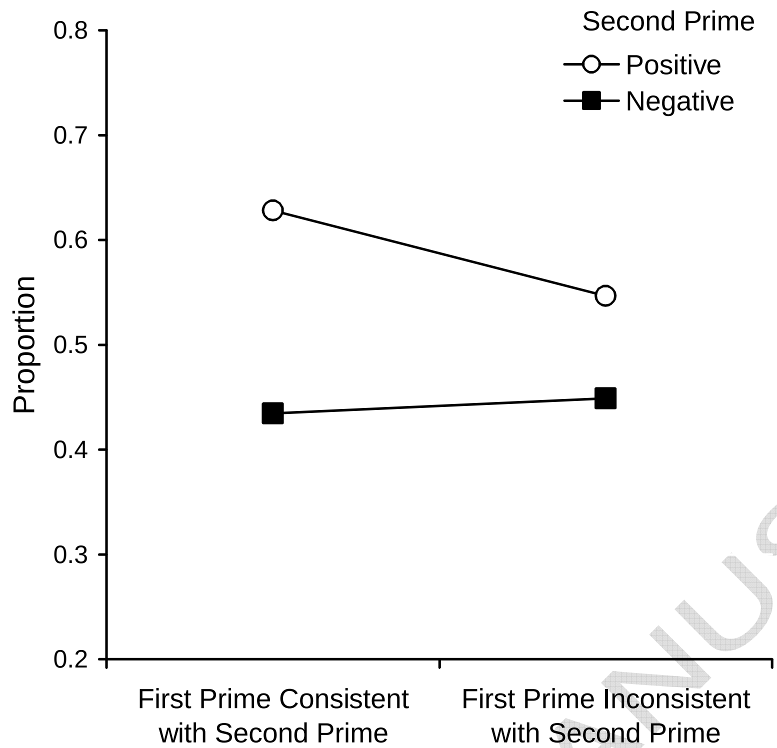

Second Prime
Positive
Negative
Proportion
0.8
0.7
0.6
0.5
0.4
0.3
0.2
First Prime Consistent with Second Prime
First Prime Inconsistent with Second Prime

Figure4

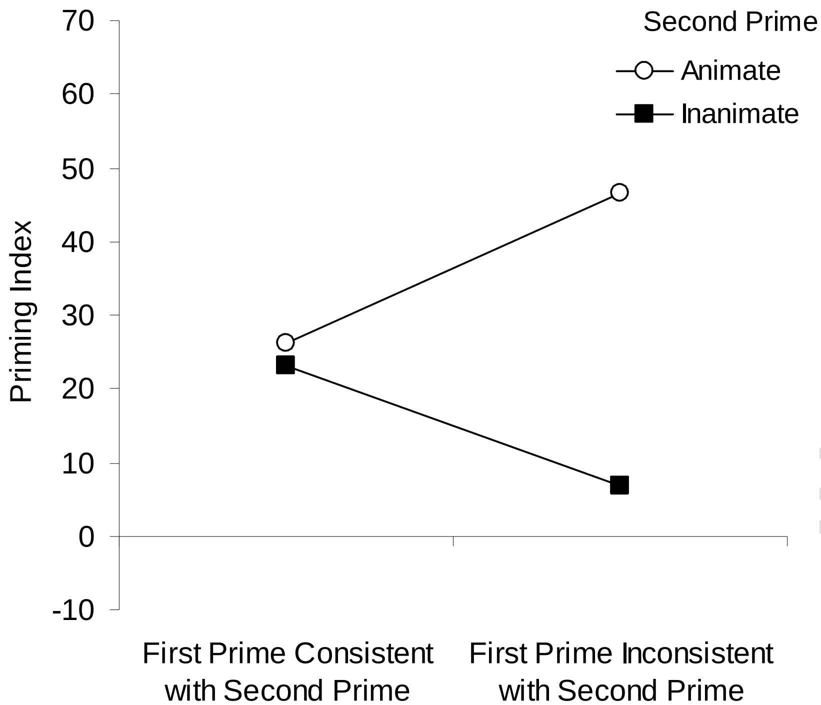
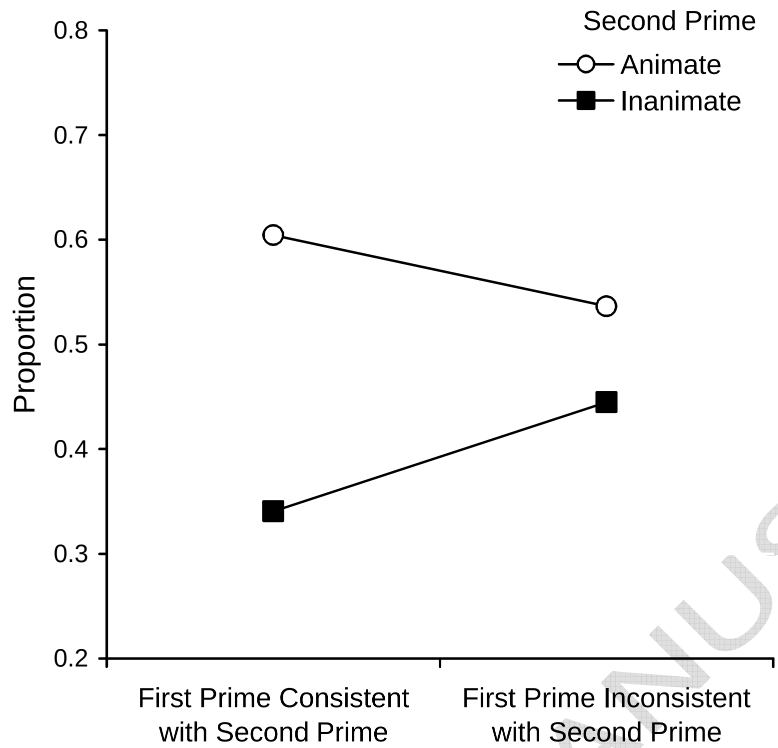

70
60
50
40
30
20
10
0
-10
Priming Index
Second Prime
Animate
Inanimate
First Prime Consistent with Second Prime
First Prime Inconsistent with Second Prime

Figure5


Second Prime
Animate
Inanimate
0.8
0.7
0.6
0.5
0.4
0.3
0.2
Proportion
First Prime Consistent with Second Prime
First Prime Inconsistent with Second Prime